



**Arthur B. Weissman**  
AGU Congressional Science Fellow

**Arthur B. Weissman** is AGU's sixth Congressional Fellow. He received his Ph.D. last year from the Johns Hopkins University Department of Geography and Environmental Engineering for his dissertation on the aesthetics and geomorphology of environmental change. He received his A.B. from Harvard College in 1970 and an M.F.S. degree in natural resource management from Yale University's School of Forestry and Environmental Studies in 1975. From 1975 to 1977 he was Assistant Director of the Nature Conservancy's Connecticut chapter. He has conducted research with the U.S. Geological Survey, the National Oceanic and Atmospheric Administration, and the U.S. Forest Service, and has held knifing positions at the Lesina American School in Lesina, Switzerland, at Maryland's Towson State University, and at Johns Hopkins University.



## Mn Solubility Tested in Seawater

upper several hundred meters of ocean water is unlike that of other trace metals such as copper, zinc, cadmium, and nickel. "Trace metals are needed as a part of 'plumton supplement' by marine plants and animals; the surface supply is biologically scavenged and regenerated at depth. This ocean concentration of trace metals increase with depth.

Manganese, by contrast, appears to be concentrated in the upper 100 m of the water column, relatively depleted in the depth interval 60-100 m from the surface. W. Landing and K. Gruland (*Ear. Planet. Sci. Lett.* 49, 45-66, 1980) described their observations of vertical distributions of manganese in a study of samples from the north Pacific. Recently, W. Sanda, S. A. Hunismann, and A. Harvey, in a study supported by the National Oceanic and Atmospheric Administration have found similar behavior of manganese in samples of coastal seawater and offshore seawater collected off North Carolina (*Nature*, 20, January 1985).

They suggest on the basis of experiments conducted with these samples that the marine biological community itself serves to condition the surface seawater, and with the assistance of photooxidation, cause manganese to dissolve in the water column.

It is believed that manganese survives in surface ocean seawater as a result of river or wind transport as  $MnO_2$ . Water upwelled from great depth usually is deficient in manganese, and manganese from submarine hot spring sources apparently does not contribute to the surface manganese profile, according to Landing and Bruland. Sunda et al. point out that the  $Mn^{IV}$  ion, as  $MnO_2$ , is insoluble and, to be sufficiently soluble to explain the observations, must be chemically reduced to  $Mn^{2+}$  or strongly oxidized to the permanganate ion  $MnO_4^-$ . As  $MnO_2$ , the manganese would be lost from the water column by sinking. Because permanganate production is highly unlikely, it seems logical that a process exists in the ocean, even in epiclastic water, that reduces manganese to the divalent state: The  $Mn^{2+}$  ion is highly soluble.

Sunda et al. collected their seawater samples off the North Carolina Coast at two stations. One was near the mouth of the New Port River Estuary and the other was out in

It was found by a series of experiments and subsequent analysis of dissolved manganese that humic acids greatly enhance the solubility of manganese oxide in seawater. In other experiments they demonstrated that humic and fulvic acids in river water also cause manganese oxide to dissolve. The process was observed to be strongly stimulated by sunlight, and thus it was concluded that the dissolution is the result of a photoactivated process.

Landing and Bruland arrived at much the same conclusion about the source of the manganese. In their analyses of surface water manganese, they also measured the  $^{210}\text{Pb}$  concentrations. They found, however, that the Mn cycle of deposition to the ocean surface and later removal from the water by scavenging is not generally coupled with the  $^{210}\text{Pb}$  cycle, whose surface distribution is characterized by its atmospheric input. They concluded that unlike  $^{210}\text{Pb}$ , most surface manganese is derived from rivers, estuaries, and near-surface shelf sediments. They note as well that manganese concentrations follow reduction zones in the oceans, corresponding to the oxygen minimum zones.

At the surface, though, oxygen is not at a minimum, and Sunda et al. think that the process they postulate not only is responsible for the geochemical concentrations of manganese at the surface but, in circular, is an important biological process. Deep seawater that is upwelled contains sufficient manganese to support maximum plant growth, they note.

This formulation may appear to diminish the role that science can play in determining public policy, but I think that it accurately reflects the distinction that must be drawn between science and policy making. Although science inevitably incorporates certain values, science alone can never determine how our society decides among competing or conflicting needs and goals. These can be determined only by social and political priorities. Science can, however, play a role in ensuring that decisions are made with a clear understanding of the problem and its possible consequences and likely solutions.

The challenge for a scientist in the public arena is to be faithful to both ends of the decision process, scientific and political. One must first get an accurate scientific picture of a problem, using the best scientific judgment in the selection and consideration of facts. Then one must purify that scientific picture in the political context and determine what political, economic, and social forces impinge on it. Parts of the picture may emerge more clearly or forcefully and parts may fade from view, but the original scientific picture should not thereby become distorted. Policy will emerge from all of these considerations, and if the politics on which it is based are reasonably wholesome, none of the 'facts' on which it is based should be compromised.

This combination of science and policy is well illustrated in the current issue of acid precipitation. One can easily get immersed in the complexities and uncertainties of the scientific issue in considering reaction rates, catalysts and limiting factors, transport from source to receptor, and effects on soils, lakes and biota. Although many of the critical links in acid precipitation are not yet scientifically established, the overall picture—the trend—is apparent to some extent, and both the public and its public servants are demanding "facts" from scientists to justify either a program of control or a policy of delay. It is clear, however, that scientific certainty alone can not determine a decision on acid precipitation control, for that is ultimately a political choice among values: ecological and possible health

The individual selected will spend a year on the staff of a congressional committee or a House or Senate member, advising on a wide range of scientific issues as they pertain to public policy questions.

Prospective applicants should have a broad background in science and be articulate, literate, flexible, and able to work well with people from diverse professional backgrounds. Prior experience in public policy is not necessary, although such experience and/or a demonstrable interest in applying science to the solution of public problems is desirable.

The fellowship carries with it a stipend of up to \$27,000, plus travel allowance.

Interested candidates should submit a letter of intent, a curriculum vitae, and three letters of recommendation to AGU. For further details, write or call Member Programs Division, Congressional Fellowship Program, American Geophysical Union, 2000 Florida Avenue, N.W., Washington, D.C. 20009 (telephone: 462-6903 or 800-424-2488 outside the Washington, D.C. area).

**Deadline: March 31, 1993**

effects compete with economic costs and social disruption. The scientific aspect of the issue concerns what, where, and how much to control; but the decision of whether or not to control acid precipitation and to what degree is ultimately political.

Thus, a Congressional Fellow must extend the comfortable universe of scientific knowledge into a somewhat alien realm where it must compete with other considerations on a more or less equal basis. Congress is, after all, like a vast marketplace, and politics at its best is the making of choices among the "common wealth." Certainly only by understanding—and not ignoring, the political process—can scientists hope to see public policies made in accordance with good scientific thinking.

The photoactivated process in the photic zone would retain manganese and thus help to maintain a supply of the dietary needs of phytoplankton. The process is thus believed to facilitate maximum primary productivity in the sea.—*PMB*

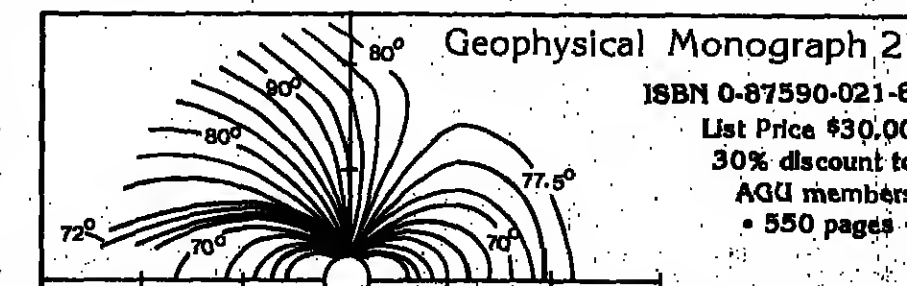
If magnetic monopoles exist at all, they may originate from a type of quantum mechanical field called a Higgs field. Higgs fields are an essential part of certain of the grand unified theories of physics, GUTs. It is nice that the so-called GUT theories can accommodate the production of monopoles in the formational processes of the universe. It is also nice that the GUT theories of particle physics can accommodate the origin of the universe at all. In a report of December's *Eleventh Texas Symposium on Relativistic Astrophysics* (*Science*, January 28, 1988), a paper by A. H. Guth of the Massachusetts Institute of Technology describes a new version of the 'Big Bang-Big Crunch' theory of the origin of the universe. Guth's theoretical model

explains a number of questions raised previously about the Big Bang expansion and, in so doing, addresses the monopole question.

As described in the *Science* report, Gai's theory uses the concept that the quantum Higgs fields can tie themselves into a knot, thus forming a heavy particle with single magnetic polarity—a monopole—having an equal chance to be of north or south character. By contrast, all known magnetic fields are directly or indirectly electromagnetic in origin and have the property that even when broken, the resulting magnetic parts are dipolar.


There are problems with producing the universe by a Big Bang, some of which involve monopolies. If monopolies can be formed, they would have done so in abundance. Monopolies should be as common in the universe as protons. Monopolies predictably would act as sort of "antigravitation" devices, and they would, if so abundant, at least have wiped out the galactic magnetic field. Further, the mass existing in our universe due to the monopolies would be huge, enough to have caused the universe to have collapsed back to its point of origin—that is, the Big Crunch idea. The Big Crunch has not oc-

News (cont. on p. 28)



edited by W.P. Olson  
(1979)

Providing an annotated list of quantitative models which serve as a reference on energy particle distribution and magnetic and electric models, this monograph was written in conjunction with the International Magnetospheric Study's activities.



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bitions of manganese in a study of samples from the north Pacific. Recently, W. Sundin, S. A. Hunniman, and A. Harvey, in a study supported by the National Oceanic and Atmospheric Administration have found similar behavior of manganese in samples of coastal seawater and offshore seawater collected off North Carolina (*Nature*, 20, January 1983). They suggest on the basis of experiments conducted with these samples that the marine biological community itself serves to condition the surface seawater and, with the assistance of photo-reduction, cause manganese to dissolve in the otherwise oxygenated zones.

It is believed that manganese arrives in surface ocean seawater by way of river or wind transport as  $MnO_2$ . Water upwelled from great depth usually is deficient in manganese

sources apparently does not contribute to the surface manganese profile, according to Landing and Bruland. Sunda et al. point out that the  $Mn^{2+}$  ion, as  $MnO_2$ , is insoluble and, to be sufficiently soluble to explain the observations, must be chemically reduced to  $Mn^{2+}$  or strongly oxidized to the permanganate ion  $MnO_4^-$ . As  $MnO_2$ , the manganese would be lost from the water column by sinking. Because permanganate production is highly unlikely, it seems logical that a process exists in the ocean, even in epic surface water, that reduces manganese to the divalent state. The  $Mn^{2+}$  ion is highly soluble.

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has developed under the influence of photosynthetic processes. The conditions appear to be favorable for the photoactivated reduction of manganese by dissolved organic matter. Manganese in this state would be dissolved, explaining observed marine profiles. The amount of sunlight and the content of dissolved organic matter decreases with depth in the ocean, so this would explain the decrease in manganese concentration. It is observed that there is an increase in particulate  $MnO_2$  concentration near the bottom of the photic zone. Such particles would sink eventually.

Landing and Bruland arrived at much the same conclusion about the source of the manganese. In their analyses of surface water manganese, they also measured the  $^{210}Pb$  concentrations. They found, however, that




face and later removal from the water by scavenging is not generally coupled with the  $^{210}\text{Po}$  cycle, whose surface distribution is characterized by its atmospheric input. They concluded that unlike  $^{210}\text{Po}$ , most surface manganese is derived from rivers, estuaries, and near-surface shelf sediments. They note as well that manganese concentrations follow reduction zones in the oceans, corresponding to the oxygen minimum zones.

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The diagram shows a series of curved lines that fan out from a common point on the left. The lines are labeled with angles: 80° and 90°. The 90° angle is at the top, and the 80° angle is at the bottom. The lines curve upwards and to the right, suggesting an expansion or growth of space.

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vices, and they would, if so abundant, at least have wiped out the galactic magnetic field. Further, the mass excess in our universe due to the monopoles would be huge, enough to have caused the universe to have collapsed back to its point of origin—that is, the Big Crunch idea. The Big Crunch has not occurred.

**News** (cont. on p. 28)

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## News (cont. from p. 97)

current, and monopoles are not uncommon, although one may have been observed (see *Eos*, February 22, 1983). Instead, the density of the universe is in critical balance between Big Crunch and infinite expansion. The remarkably precise, symmetrical, and uniform nature of essentially all of the properties of the universe seem to defy theoretical logic. To explain all these unusual phenomena, and to do so within GUTs, Guth invoked an "inflationary scenario," an exceedingly rapid expansion stage within the very first part of the Big Bang.

As argued by Guth, "For the real universe to be so close to criticality now, some 10 billion to 20 billion years after the Big Bang, it must have differed from criticality in the earliest instants by less than one part in  $10^{16}$ ." An initial condition arises not for an explanation. The explanation fell out of a sort of quantum phase diagram approach. In a particle physics analogy to a crystallization sequence, the whole universe undergoes a series of phase transitions. The Higgs field "freezes out" in this model and in turn forms a field of lattice points that affects quantum particles and their mutual interactions. The way the Higgs field freezes in those models is rather important, the results producing a range of possible structures from a sponge-like character in that of a condensed mass of jelly. The "jelly" model yields the best results. In its scenario, the universe begins at a point source whose diameter is one trillionth that of a proton. The inflation begins, therefore, from a system small enough to have achieved thermal equilibrium. The inflation undergoes a unifying process resulting in a uniform universe. The monopoles, or most of them, were lost in the phase transition stage that changed unidentifiable elementary particles such as quarks, leptons, and neutrinos into composites such as protons, neutrons, or mesons. Electrons and neutrinos stayed in the free state as they are observed now.—PMB

## Study Finds Carbon Mobility in Olivine

The significance of carbon's existence as a solid solution in olivine is many fold. Currently there are speculations about the contributions of dissolved carbon to the electrical conductivity of minerals that constitute the earth's upper mantle, and for this contribution olivine is a prime candidate. If carbon, once dissolved, is relatively mobile in upper mantle mineral assemblages, a lot can be speculated about diffusional processes in the upper mantle, the very diffusional processes many geophysicists ordinarily assume to be relatively unimportant. Diffusion—creep processes that support convective overturn in the solid state—can be accelerated, perhaps, by the mobility of minor elements.

In their recent study of carbon in natural olivine, G. Oberheuser, H. Kahr, G. Demortier, H. Gonska, and F. Freund of the University of Köln, West Germany, found that the carbon in olivine exists in a truly dissolved state, not as carbon dioxide related anionic groups or as a graphite-like polymer (*Geoch. Cosmoch. Acta*, in press, 1983). They found that the dissolved carbon has a relatively high diffusion rate which is, needless to say, unexpected for an olivine host. Their study showed that the carbon was not associ-

ated with major lattice defects such as dislocations or subgrain boundaries. The carbon apparently is bonded with the olivine with an approximate C-O<sup>2-</sup> character.

Oberheuser et al. used two specialized methods to study the carbon. They analyzed the carbon by means of observing the nuclear reaction <sup>13</sup>C(p, n)<sup>13</sup>N, and also by the X-ray induced photoelectron spectroscopy (XPS) technique. Carbon analysis by nuclear reaction is a rather novel approach, described by Oberheuser et al. as being "a powerful method of certainty of interest to many petrologists." The beam of a particle accelerator, deuterium ions in this case, accelerated to an energy lying below the Coulomb barrier, is impinged on the sample. The deuterons convert <sup>13</sup>C nuclei to <sup>13</sup>N and in so doing lose energy as a function of depth in a process that can yield depth concentration values based on standardization. Due to interferences from other element reactions with light elements, the useful depth range of analysis of carbon in olivine is limited to about 2.5 μm. To observe kinetic, diffusional parameters, the changes from low temperatures (liquid nitrogen) to high temperatures (1050 K).

The XPS or ESCA technique used in this model and in turn forms a field of lattice points that affects quantum particles and their mutual interactions. The way the Higgs field freezes in those models is rather important, the results producing a range of possible structures from a sponge-like character in that of a condensed mass of jelly. The "jelly" model yields the best results. In its scenario, the universe begins at a point source whose diameter is one trillionth that of a proton. The inflation begins, therefore, from a system small enough to have achieved thermal equilibrium. The inflation undergoes a unifying process resulting in a uniform universe. The monopoles, or most of them, were lost in the phase transition stage that changed unidentifiable elementary particles such as quarks, leptons, and neutrinos into composites such as protons, neutrons, or mesons. Electrons and neutrinos stayed in the free state as they are observed now.—PMB

Total carbon was measured in the olivine crystals by means of an ASTM standard-type carbon analyzer to be on the order of 180 ppm in one specimen and 60 ppm in another. The heating-cooling cycles produced rather sharp profiles of carbon as analyzed by the nuclear reaction and the XPS methods. That the high mobility of carbon observed was due to a thermally activated diffusional process was demonstrated in part by the reversibility. The highly mobile carbon species in these olivines could be made to diffuse to the surface, and then back into the crystal interiors. The driving force for this process was thought to originate in localized lattice strains of carbon atoms in the olivine crystal matrix. Carbon-oxygen bonds would be dipolar. The carbon species would be driven differentially toward classically relaxed volumes. The diffusivity equation was evaluated by substituting the measured values obtained by both techniques as follows. The equation is given by

$$D_{\text{carbon (olivine)}} = D_0 \exp(-E/RT)$$

where  $D_0$ ,  $E$ , and  $R$  are the frequency factor, the activation energy, and the gas constant, respectively. From the nuclear reaction data obtained at  $T \approx 300$  K:

$$D = 10^{-12} \exp(-7.8/RT)$$

and from the XPS data obtained 450 K  $\leq T \leq 925$  K:

$$D = 10^{-14} \exp(-6/RT)$$

(both in  $\text{m}^2 \text{s}^{-1}$  and  $\text{cm}^2 \text{s}^{-1}$ ). The activation energies are unusually low for the case of carbon atoms diffusing through a relatively dense packed crystal structure such as olivine. It would appear that if correct, the data imply a great mobility of carbon in olivine.—PMB

## Can Tests Identify Creative People?

It is always a popular pursuit by a number of administrators to assess the creativity or innovative qualities of scientists in order to evaluate their research capabilities. Of course, traditionally such evaluations have been fraught with subjectivity (to be weird, under 40 years old, independent, risk-taking, etc.), and such evaluations have not been highly valued.

In recent years, through testing, the American Chemical Society (ACS) has attempted to give respectability to the art of judging the creativity of a scientist. ACS, which draws its members from both industrial and academic laboratories, held a symposium on the subject of evaluating the creativity of scientists. The proceedings were published by ACS as "Innovation and U.S. Research: Problems and Recommendations" (W. N. Smith and C. F. Larson, eds., 1980). In the proceedings, as reported in the July 1982 *Chemist* (all quotes here are from the *Chemist* article), A. Nissen was able to identify only the following two-part characteristic of an innovative person: (1) a low threshold to a state of discomfort with some aspect of the order of things, the status quo; and (2) an extraordinarily high level of mental stamina enabling him or her to persist until the state of discomfort is removed.

W. S. Lyon of Oak Ridge National Laboratory (ORNL) has evaluated the results of new tests and has concluded that, "a simple, reliable measure of creativity seems to be simply to ask the person." He qualifies this by pointing out that, "Such an interrogation must probably should be casually put among other questions, perhaps in written form, so that the respondent does not feel obliged to maintain face by answering 'yes'."

Lyon's idea was to compare the results of two creativity tests, which he named after their authors as the 'Davis' and 'Ransdopp' tests. Fifty-eight scientists in ORNL's Analytical Chemistry Division took the two tests, 6 months apart. They were also asked to answer four self-evaluation questions and to make peer evaluations. No mention of creativity was made, however. According to Lyon, "We think it important to emphasize that the questionnaire was not presented as a test of creativity. It is possible that 'creativity test' such as the Ransdopp test automatically sound an alarm in the minds of reasonably intelligent people, an alarm that, if as they may be objective, will still cause them to pick the obvious 'creative' answer."

Lyon compared the results of the two creativity tests to the self-evaluation questions and the peer evaluations. Lyon found good statistical correlation between the test and the self-evaluation questions, lending support to the idea that 'most creative people recognize their own creative abilities.' The results also showed good correlation between high creativity scores on the tests and peer judgment, and fair to good correlation between test scores and the number of talks and publications of the scientists questioned. On the issue of any presumed relationship between creativity and age, Lyon said, "Essentially, we found no correlation between score (creativity) and age or length of service of respondents."

The top quartile, the most creative, seemed to be occupied by Ph.D. holders of average age 48 and the bottom quartile, the least creative, by non-Ph.D. scientists of average age 58. The Ph.D. and age factors, if taken separately, show no such correlation, however. Not only does measured creativity have no correlation with age but, as Lyon puts it, "Creativity, as measured by the test, is no respecter of academic degree."

The way of testing at ORNL appears to be useful in evaluating the creativity of individuals within groups having very high or very low creativity. The test did not, however, examine factors that may enhance the creativity of a presumably creative group of people such as scientists.—PMB

## Geophysicists

William W. Fox, Jr., has been appointed director of the Cooperative Institute for Marine and Atmospheric Studies (CIMAS). He had been director of the Southeast Fisheries Service since 1978. CIMAS was established in 1977 by the National Oceanic and Atmospheric Administration and the University of Miami. Seven of the 889 U.S. Fulbright Scholars for 1982-1983 are lecturing and conducting advanced research in geology in universities abroad. Brian Francis Farrell, a research assistant in planetary studies at Harvard University, is lecturing in oceanography at the University of Cambridge in England through June. William B. Ferguson, associate professor of civil engineering at Villanova University, will lecture in geology at the Xengwon National University in Korea until July. Ray Edward Farrell, Jr., geology chairman at Louisiana State University in Baton Rouge, lectured and conducted research in marine

## Forum

## Joint AGU and Lunar Science Conference

For the past 3 years there has been increasing concern about the duplication of papers on lunar and planetary sciences between the meetings of various societies and the annual meeting at the Johnson Space Center, Clear Lake, Texas. In addition to the costs for travel and accommodation (approximately \$400-\$800 per conference for most attendees from the United States) there is the time spent on all planes and sitting in sessions. I argue that a potential saving of at least \$300,000 per year justifies a reduction of meetings on lunar and planetary sciences. We must find money for young scientists and new ideas.

I propose that the 15th Lunar and Planetary Science Conference (LPS-C) be held in association with AGU's 1984 Spring Meeting. Sessions of general interest to all AGU members would be scheduled mainly on Thursday and Friday of the regular AGU session, whereas sessions and workshops of special interest would be scheduled on Saturday and Sunday. The usual abstract would be submitted for publication in *Eos* by those lunar and planetary scientists who wish to be considered for the sessions of general interest. The AGU program committee would select abstracts for the general session and turn over the specialized abstracts to an LPS-C committee for scheduling on Saturday and Sunday. The Lunar and Planetary Science volume of 2-page abstracts could be prepared as usual and distributed only to those who specifically pay an extra registration fee.

Of course there are many considerations which I would make it difficult to reach a consensus on the move I am proposing: one in particular would be the fear that the Lunar and Planetary Institute might lose some status and power if the conference moved from Clear Lake. Nevertheless, I believe that this plan offers an opportunity for integrating planetary sciences and meteoritics into a broader cosmological and cosmochemical context as well represented by the American Geophysical Union. This should help to strengthen the constituency for planetary sciences and meteoritics; in particular, the current, alarmingly poor state of scheduled planetary sessions might be improved with a strong push from the entire AGU community. Such a long-term gain would offset my immediate losses that might be apparent to one of more groups of planetary scientists. Furthermore, the planetary scientists could help to strengthen the AGU Spring Meeting. There would be problems but I believe that they can be overcome.

What is your opinion, ladies and gentlemen of the AGU community and particularly of the planetary section? Can you suggest a better alternative?

Joseph V. Smith  
University of Chicago

geology at the University of Oslo in Norway. M. Allan Rags, professor of geology at the University of Oregon in Eugene, will conduct research in geology at the University of Copenhagen in Denmark through April. Robert Vernon McGee, associate professor of health education at Southern Illinois University (University Station campus), will be lecturing in geology at the University of Monrovia in Liberia through July. Bruce Warren Nelson, a professor of environmental studies at the University of Virginia in Charlottesville, will be lecturing in geology at the Universiti Malaya in Malaysia through April. Ronald Porter Willis, professor of geology at the University of Wisconsin-Eau Claire, will be lecturing in geology at the Seoul National University in Korea through July.

## In Memoriam

The following AGU members are recently deceased:

John C. Hagan, 78; AGU Life Member, joined the Meteorology Section (now Atmospheric Sciences) in 1939; died January 17, 1983.

David Kiehn, 31; joined in 1982 as a student member of the Volcanology, Geophysics, and Petrology Section; died December 11, 1982.

E. J. Workman, 83; AGU Life Fellow; joined the Meteorology Section (now Atmospheric Sciences) in 1942; died December 21, 1982. A scholarship has been established in his name at the New Mexico Institute of Mining and Technology, where he was president and director of the research and development division for 18 years.

## Books

## Cosmic Electrodynamics

J. H. Piddington, 2nd ed., R. E. Krieger, Publ., Malabar, Fla., xii + 361 pp., 1982, \$27.50.

Reviewed by C. T. Russell

This highly readable monograph is an extensive revision of an earlier book published in 1969 by John Wiley. Piddington clearly loves his subject matter and writes with enthusiasm about the role and critical importance of magnetic fields in the universe around us. He treats solar magnetic fields, the interplanetary medium, the magnetosphere, planetary magnetospheres, and galactic magnetic fields. The treatment is far from uniform, and the topical balance has shifted somewhat from that in the first edition to that in the second. For example, in the first edition there were 27 pages devoted to solar magnetism, and in the second 81 pages. In the first edition there were three chapters devoted to geomagnetism and in the new edition two.

Piddington takes the reader on a whirlwind tour of the cosmos in a more qualitative fashion than quantitative. The author appears to have wanted to document his understanding of the subject and the reasons for his point of view rather than to write a text book. However, with a moderate amount of auxiliary material this book could serve as the focus of a course. Alternate hypotheses are mentioned throughout.

The section on magnetospheric physics, both terrestrial and planetary is quite out-

dated in part because of the explosive growth in our understanding of the magnetosphere of the earth with the ISEE mission, of the solar wind-Venus interaction with the Pioneer Venus mission, and of the magnetospheres of Jupiter and Saturn with the Voyager spacecraft. For example, islands of energetic particles in the tail are mentioned on page 242, whereas we understand such observations today as simply expansions and contractions of the plasma sheet. No mention is made of flux transfer events, inverted V's, conics, vortices, or other important features of the terrestrial magnetosphere. While much is said about flux ropes in the sun, nothing is mentioned about flux ropes in the Venus atmosphere where they have been probed in situ with the Pioneer Venus orbiter. Piddington recognizes this problem in the preface of the first edition where he states, "A monograph dealing with such a fast developing subject tends to grow out of date." This statement is still true in 1983 but it is exacerbated by the fact that the book was apparently written in 1977 to judge by the date of the most recent references.

Nevertheless, there is much to recommend this book. It is well written and treats a very broad subject area, often with keen insight. It is recommended reading, especially for those interested in solar magnetism and Piddington's rather unorthodox views.

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## Scientific Basis of Water Resource Management

Geophysics Study Committee, Staff, in Geophysics, National Academy Press, Washington, D.C., xii + 127 pp., 1982.

Reviewed by H. J. Morad-Seydoux

The least that one can say about the report is that it is very carefully written. Every chapter has been carefully written, and the literary style of some chapters is outstanding (particularly those by Klemes, "Empirical and Conceptual Models in Hydrology," and by Baker, "Geology, Determinism, and Risk Assessment"). The best that one can say about the report is that it does meet its stated objectives: (1) evaluation of the adequacy of present hydrologic knowledge and of the appropriateness of present research programs to provide information for decision making and (2) description of the impact of hydrologic knowledge on the planning and management of water resources. The report is not particularly original and that there are few really fresh new arguments developed in it. One unimpressive exception is provided in Chapter 11, by Matias, Landwehr, and Wolman, which challenges the traditional (implicit) assumption that "human activity is an external perturbation of the hydrologic cycle." Though not the explicit intent of chapter 4, by Bredehoeft, Papadopoulos, and Cooper, with the explosion of the water-budget myth in groundwater, this chapter illustrates clearly the profound interaction of man (through wells) in the hydrologic cycle, a situation that cannot be comprehended from a study of the system free from human influence.

There is a consensus in the report that there is currently much misguided research. Much hydrologic research is directed at problems that are not necessarily the most significant ones in theory, on the ground, or in practice (Leopold, p. 107), or are "scientifically sterile" (Klemes, p. 99). "Mathematical convenience is a popular refuge, current fashion running a close second" (Klemes, p. 99). Practically, all authors call for a return to a search for basic understanding of the dynamic mechanisms governing the processes (theory), a return to experimentation (not to be confused with the "simple collection of data that sometimes passes for field hydrology" (p. 28)) and the abandonment of computer-induced model building, conceptual or statistical, with fruitless overgrowing mathematical sophistications as a research goal in itself. The new focus for research will require an uncommon degree of cooperative endeavor from a broad range of the earth sciences (Eagleson, p. 30) in which water quality will become a predominant concern.

Chapter 1 (Dunne) presents an excellent state of the art for runoff processes. Chapter 2 (Eagleson, "Hydrology and Climate") is interesting, but not too exciting (though 8 is fully justified). Chapter 3 on the vadose zone (by Nielsen and Biggar) is good, but the authors recent crash for the stochastic approach may well lead promptly to the blind alleys against which Cassandra warns (in chapter 8) on empirical and causal models by Klemes. Chapter 5 (on water quality by Slevin and Sumner) is also good, but the authors seem also tempted by the devil of "reductionism." Chapter 6 (on aquatic ecosystems by Cairns) is interest-

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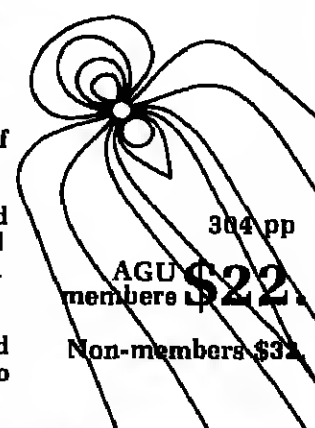
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C. T. Russell and D. J. Southwood, editors

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ing, but out of place in this report. The philosophy expressed in chapter 7 ("Robust Estimators in Hydrology," by Fiering and Kuczera) is remarkable, but, most ironically, the authors fail immediately, in their very own chapter, into the pitfalls against which they warned: they use the "model" building on p. 91 and the "reductive" (cf. Figure 7.2). Chapters 9 (Leopold) and 10 (Baker) on hydrology and geomorphology go together. They are to the point and refreshing. What better way is there to denounce the fruitless efforts of developing better planning position formulae (as an example of futile research) than to take a look at Figure 10.3. Chapter 11 on prediction is a surprise. After all, there is no harm, indeed some wisdom, in concluding (even if only implicitly) that one may have been wrong in the past. Only outstanding scientists can afford such turn around and lead again in a new direction.

At the Ph.D. level, and maybe at the M.S. level as well, the report should be required reading for all graduate students (and professors) specializing in hydrology. In a course on Physical Hydrology, as an antidote to brainwashing, Chapters 7 and 11 should be required reading, whereas for Stochastic Hydrology and/or Conceptual Hydrology, the reading list would include the chapters 1, 8 (of course), 9, and 10.

It is a worthwhile report. Read it.

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## The Mineralogy, Chemistry, and Physics of Tropical Soils With Variable Charge Clays

G. Uehara and G. Gillman, *Westview Trop. Agr. Ser.*, Westview, Boulder, Colo., xviii + 170 pp., 1981, \$30.00

Reviewed by D. K. Cassel and D. R. Nielsen

This book is the culmination of an effort started in 1974 when the senior author started assembling information for a tropical soils course that he taught while on sabbatical leave at North Carolina State University. The literature cited throughout the book was current when the book went to press.

Soil systems contain mineral and organic materials that have consistent or permanent surface charges, such as montmorillonite, or constant surface potentials, usually referred to as variable charge materials. Most soil systems contain some of both kinds. In the tropics, most of the minerals with permanent charge have been severely weathered. Consequently, the surface charge of the remaining material results from adsorption of potential determining ions. This book treats the mineralogy, chemistry, and physics of the variable charge minerals and soil organic matter.

After a brief introduction, chapter 2, entitled "Mineralogy," discusses the relationship between weathering and mineralogy. Mineralogy is currently used to classify soils at the taxonomic family level. The authors go one step further and present a schematic that shows how, in general, the soil orders relate to mineralogy. For example, Vertisols consist primarily of permanently charged smectites. Oxisols consist primarily of oxides and hydroxides, and Andisols are composed primarily of noncrystalline materials.

The main emphasis of the book is placed on the chemistry of constant potential surfaces. Chapter 3, entitled "Chemistry," com-

pleted with chapter 6, entitled "Analytical Methods," take up 81 of the 157 pages. The physical chemistry is reviewed for both constant charge and constant potential than a system containing both kinds of clay is discussed. Heavy emphasis is placed on the zero point of charge and how it relates to soil pH. An example explains the theory associated with rejuvenating an acidic soil in Brazil by liming (adding divalent Ca and Mg cations to replace monovalent H ions) and adding a heavy application of phosphorus (increases the net negative charge of clay surfaces and increases the retention of cations such as phosphorus).

The 30 page chapter on physics is disappointing. Nearly all facets of physics are mentioned—rheology, soil water, solute transport, soil temperature, and soil air—but with little depth. In general, this book summarizes some of the soils consisting predominantly of constant potential materials. The examples that relate agricultural management practices to this chemistry make it worthwhile to read. The book is recommended to provide background information for the student who has little previous knowledge about the theory of constant potential surfaces and for the scientist who is not familiar with differences in chemistry between constant potential and constant surface charge soils.

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